October 11, 1950.

Dear Bernie-

First, I want to thank you for arranging for Travis-Farber to send the Incabloc. We just received it, and have remitted for it. Could I ask for a repetition of the favor? Both Esther and I have had a lot of trouble with timepieces lately, and we would both like to be better equipped. I am enclosing a check for the same amount as the previous billing -- but if there is any delicacy about it, please just tear it up.

Second, have you forgotten about sending me a stack of mutants of K-12? You talked as if they had already been sent out, and I wonder if they can have been lost in the mails.

Lately, I've been looking for reisolations of K-12. In order to minimize the manipulation of test cultures, I tried crossing W-1177 ($Thr^-B_1^-$ S^r) with K-12 or other prototrophs on minimal streptomycin medium, which selects, of course, for X^+S^r recombinants. The S^r mutation is too infrequent to cause trouble. The method works like a charm-- for K-12. About 40 other E. coli and 100 Salmonella cultures so tested gave negative results. Two or three E. colis have been picked up, however, which seem to recombine with K-12, but at a very low rate (ca .001 compared to K-12). It is not even necessary to wash the mixed cultures from broth before plating, as the streptomycin prevents syntrophism and the residual growth of W-1177 does not interfere. Would you send me E. coli W for retest this way? If you have any other coli strains lying around, I'll be happy to try them too.

Since coming back, I've done a few experiments with Pseudomonas fluorescens A3.12 (Stanier's bug). It works like a charm in the penicillin method, although the unitage has to be stepped up to 1000/ml. Currently, I'm trying to find mandelate-negative mutants with indifferent success. Have you tried penicillin for carbon source mutants in E. coli? My immediate interest has been in checking for recombination, but the results are negative, or, at best, inconclusive.

Nothing else much now, except a remarkably mutable Mal- (or is it Mal^+) mutant after UV which at first looked like a very peculiar type of segregation. But the cytology and further genetic analysis agreed in throwing out this notion. It has a mutation rate from Mal^+ to - that I guess at about 10^{-2}, and the reverse somewhere around 5×10^{-4}. So far, the culture has oscillated back and forth; no stable + or - have been found. This is the only very mutable culture I've seen so far, and fermentative mutants of this kind could not be missed in our standard procedure. Have you seen anything like it (except material like Bunting's Serratia & colonial variations)?

Sincerely,

Joshua Lederberg